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THE NATURE OF SPACE. II

THE EMPIRICAL BASIS OF GEOMETRY

IT is so long since geometry reached the assured position of a deductive science, that it is hard for us now to realize that it was ever in an inductive stage. But this, of course, is plain historical fact. The geometry of the Egyptians was a body of practical rules of mensuration, which were the outcome of long experience and careful observation—though they were far from exact, according to our standards. And the geometry of the early Pythagoreans, while it exhibits from the outset the Greek interest in pure theory, and was thus destined to develop into the organization of consecutive demonstrations which the science in its classical form exhibits, was still essentially empirical in its methods and standards.

When one speaks in this way of an inductive and a deductive stage in the development of a science, it is not suggested that in the former stage deductive processes are not employed. The distinction rather is that when the deductive stage has been reached induction is no longer used as a method of proof, but only for purposes of discovery or of illustration. When it has been demonstrated, for example, that the three internal angle-bisectors of a triangle meet in a point, there is no necessity for confirming the result with rule and compasses; and if, on actual trial, the three bisectors should appear not to converge, one would blame the draughtsman or his instruments, not the principles involved.

In the case of geometry, the deductive stage waited upon the conception of the *point*—of that which has position and not magnitude. The existence of bodies must be attested, directly or indirectly, by specific observations. The existence of points is attested by general axioms, which recognize no distinction between possible points and actual points. Take away all bodies, and the system of points is conceived to remain the same. Thus the system of points is space *as such*—space which may be full or empty without its characteristic properties being in any way affected.

The earliest Greek geometers knew nothing of the point: and accordingly they could not make the distinction between space and extended matter. The folk-mind, as well as early science, has the two conceptions, "something" and "nothing," the former of which is often identified by science with "matter," the latter with "empty space." But the latter identification, at least, is inexact. Empty space can be conceived only as distinguished from *filled space*, not as distinguished from matter; for space is space, whether filled or empty. The folk-mind also has a conception of "place"—a place where something may or may not be. And it has the closely related conception of a "hole" which may be made in a given place, and which again may be filled up. These also are not equivalent to the notion of space, because of the relativity to actual "somethings" which they contain. A place in a moving cart moves with the cart. A hole in a log disappears when the log is burned up. If there is an unmoved place, it is at, in, or on something that is unmoved. The developing scientific consciousness appropriates these conceptions, and interprets them in the light of its space-conception. For science, it is primarily the point that has position; and the position of a point is primarily relative only to other points. The hole, if really unfilled, becomes a "vacuum," *i.e.*, empty space; and, contrariwise, infinite space itself becomes the "universal receptacle"—a hole, if you please, that is not a hole in anything. But these are developments that distinctly belong to science, not to the folk-consciousness, and not even to the beginnings of science.

The Pythagoreans long confused mist and darkness with empty space. And, at the same time, they thought of space (or mist) as made up of discrete units. Two of these units, side by side, made the shortest possible line. Three were necessary to make a surface. A little pyramidal heap of four was the minimum solid.

What is geometry without the point? And how can such a geometry give rise to the conception of the point? These are the questions which are now to engage our attention. Unfortunately we can have but slight guidance from the recorded history of the science in answering them. The fundamental principles that are involved are such as must have come to universal recognition long before the beginnings of geometry as a distinct body of knowledge. Accordingly, they are not regarded by primitive scientists as worthy of explicit statement; and when geometry reached a stage in which the systematic statement of its foundations was felt to be a desideratum, the conception of the point, with the attendant abstraction of space from matter, was already firmly established, and no need was felt for going behind it.

The foundations of an inductive science differ necessarily in the most radical fashion from the formal beginnings of a deductive science. The latter has its few indefinables and its set of axioms. The inductive science has indefinables in plenty, not in the sense of concepts that do not need definition, but in the sense of concepts that are too vague to admit of definition; and it has unproved principles in plenty, which are assumed, not because they are self-evident, but because they are plausible. As the science advances, its earlier indefinables may be defined, or they may disappear by reason of radical inconsistencies that have been discovered in them; and, at the same time, the earlier "axioms" may be demonstrated, or they may be shown to be unsound. "As far as the east is from the west," said the psalmist, little thinking that east and west come together. That bodies which are not supported fall downwards is an ancient maxim, from which even today our imaginations are not wholly liberated. With the earliest physicists it sometimes passed unquestioned; witness, for example, Anaximenes's theory, that the reason the earth does not fall is that its shape is that of a broad leaf, so that it is supported by the air.

Starting from such assumptions, the inductive science attempts to reach clearness in some one direction, leaving everything else untouched for the time being. Such attempts are generally destined to meet with very slight success; for the assumptions that are still blindly made vitiate the whole procedure. But there is no other way. We can not move our whole thought-world at once. It is only by using the solid earth of common sense as a fulcrum that we can hope to budge some one block of prejudice. It is only by trusting to the great mass of our opinions as if they were absolute knowledge, that we can hope to correct some special misconception. Success comes when, by good fortune, the unsteadiness of our fulcrum does not markedly affect the work performed.

Let us enumerate some of the common-sense conceptions which inductive geometry must use in order to give clearness to its own conceptions.[1]

First in order stands the physical *solid*. In some rough fashion this conception must have belonged to men since they began to reflect at all. Solids behave in characteristically different ways from liquids, as well as from wind and fire. They impress us differently, and we manipulate them differently—in just what ways we need not specify. Nor need we stumble over the fact that there are all degrees of viscosity and solidity that separate the typical liquid from the typical solid; much less distress ourselves with the thought that not even the typical solids are absolutely rigid, even when they are not long and thin enough to be bent like wire. We put all such sophistications aside; and, without pretense of definition, we boldly assume that we mean something definite enough by a "solid," and that solids in general are good enough solids for the immediate purposes of our science.

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[1] A first sketch of the theory here developed is contained in an article on "The Nature of Primary Qualities," in the *Philosophical Review*, Vol. XXII, pp. 504–6.

Secondly, there is *motion,* with its negative *rest.* We have a variety of ways of perceiving motion, both in our own bodies and in other bodies. As little as in the case of the solid shall we attempt to be precise or to guard our conceptions from ambiguity. We shall not ask ourselves whether by "motion" we mean absolute or relative motion, or, if the latter, relative to what. No; we simply mean motion, as we see it and feel it. Again, we do not allow ourselves to be perplexed by the image of an Heraclitean flux; nor by the thought that, as observation becomes more exact, many things that seemed to be at rest are found to be in motion, so that it is doubtful whether in the world anything is at rest either absolutely or relatively to anything else. We ignore all such considerations. By rest we mean the condition of most of the things about us, most of the time, as they appear to common observation.

Thirdly, there is *contact.* This also we have various ways of perceiving, not all of which are always possible, but which generally confirm one another. We can often see that two solids are in contact with each other, and hear when they are brought into contact. We have a special sensation that informs us when something touches our own body; and, by a change in its intensity, this sensation also informs us when a solid which we are holding touches another solid. Strain sensations serve the same purpose. There is also this familiar and fairly trustworthy test: that it is only when two solids are in contact with each other or are both in contact with a third (intervening) solid that we can cause a movement in one of the two by moving the other.

Lastly, there is *simultaneity,* whether it be of momentary events or of more or less enduring conditions. However, on account of the narrowness of the field of human attention, simultaneity can, in general, be attested only by an absence of relevant change as our observation shifts to and fro between the compared objects, and hence can, in general, mean only the temporal overlapping of conditions.

How are these conceptions involved in geometrical observations? The answer is not far to seek. Spatial measurement is an operation performed primarily upon solids. To measure a liquid or a vapor is, in general, to measure its solid container. Moreover, the measuring-rod itself is in all cases a solid, either rigid in itself, or—as in the case of a taut string—given a temporary rigidity. Geometry, as the science of spatial measurement, is thus a science of solids; and, while endeavoring to refine its conception of certain of the properties of solids, it must necessarily take for granted at the outset a gross common-sense notion of the solid as such.

More precisely, the operation of measurement, in its fundamental form, involves the observation of the occurrence or non-occurrence of the *simultaneous contact of one solid with two others,* which may, indeed, be parts of a single solid. There are, of course, other modes of measurement: directly by the eye or hand; indirectly by the time which a familiar movement, such as walking, over the given distance requires. Or a measuring-rod can be used without actual contact. But, when we observe that one solid touches two others simultaneously, we know, with an assurance limited only by our confidence in that observation, that the distance between the two is not greater than the length of the intervening one. And when we find that, try as we may, we can not make one solid touch two others at once, we are assured that the distance between these two is greater than the length of the one. For the purposes of empirical geometry, that is the meaning of these observations.

In all this it is assumed that our powers of manipulating the solid with which we try to touch two others are practically unlimited; and, in particular, that we can freely move any fourth body that might otherwise interfere with the process.[2] As a matter of fact our ability freely to manipulate objects, or to move them so that they will not interfere with our manipulation of other objects—or, for that matter, to keep them at rest while other objects about them are moving—is decidedly limited. We must, of course, base our inductions on the observations where the required conditions are satisfied; and we then assume that were our powers greater we should have been able to make similar observations in the other cases. In this respect empirical geometry does not differ from other empirical sciences.

Just as men may be assumed to possess, from the earliest stages of reflection, some notion of the solid, of rest and motion, of contact, and of simultaneity, so they may equally be assumed to possess some notion of *distance* and of *length.* We can not, however, in what follows make any express appeal to this phase of early thought; for it is precisely here that our endeavor to substitute clear definition for common sense must begin.

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[2] Here, in addition to the conceptions enumerated above, a vague conception of *force* is involved.

The first step is to bring before our attention a certain fundamental induction, which may be called the *principle of measurement,* and upon which our whole further procedure rests:

If the solids *A, B, C,* and *D* are at rest, and if there is at least one solid *X* which can be brought into simultaneous contact with *A* and *B,* but can not in any way be brought into simultaneous contact with *C* and *D,* then there is no solid that can connect[3] *C* and *D* but can not connect *A* and *B.*

Attention must be called to the condition that no one of the four solids, *A, B, C,* and *D,* shall move. If any one of them is observed to move, further observation often shows that it is then possible to find a solid that can connect *C* and *D* but not *A* and *B.* But, furthermore, sometimes it happens, even when no one of the four has been *observed* to move, that nevertheless the principle fails to hold. In that case, we say, perhaps, that one of the objects actually moved, though we did not observe it, either because we were not watching it at the time, or—and this is the important point—because our observation was defective. We are often ready, in such a case, to condemn an observation as defective, even though we have no reason to believe that it was less careful than any observations upon which we impose complete reliance. Or we say that in the interval between the tests some change occurred in the capacity of one of the manipulated solids to connect other solids.

Thus this fundamental induction of geometry is not supported by evidence that is in itself perfectly consistent. Direct observation does not prove it to be invariably true, but rather goes to show that it is sometimes, though comparatively rarely, contrary to fact. We follow in the induction the overwhelmingly great mass of our observations; and we simplify our experience by bringing the apparent exceptions into line. The simplification is the greater, in that we are thus enabled to extend our induction to *all solids whatsoever,* whether they be at rest or not. It now becomes:

If *A, B, C, D,* and *X* be any solids, and if, at any moment, *X* can connect *A* and *B* but can not connect *C* and *D,* then, at that moment, no *Y* exists which can connect *C* and *D* but not *A* and *B.*

The proposition at which we thus arrive may be brought under a wider induction that is reached as follows:

In the first place, if there exist two solids, *X* and *Y,* such that while *X* touches *A,* and *Y* touches *B,* they can at the same time be made to touch each other; and if *X* and *Y* can not be made in this sense to "connect" *C* and *D;* then no solid, and no pair of solids, can be found that can connect *C* and *D* but not *A* and *B.*

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[3] We shall hereafter use this term instead of the cumbersome phrase, "be in simultaneous contact with."

At the same time, let us note that if X and Y can connect A and B in the order, "A touches X, which touches Y, which touches B," X and Y can also be made to connect A and B in the order, "A touches Y, which touches X, which touches B."

In the second place, if there exist one or more solids, V, W, X, Y . . . ; which can be made to connect A and B in that order, then they can be made to connect A and B in any order whatsoever. And if, further, they can not be made to connect the solids C and D, then no solid or chain of solids[4] can be found which can connect C and D but not A and B.

In the third place, where there are more than two solids inserted between A and B, it is not necessary in our tests that all the contacts be simultaneous. For example, let V touch A and W; then, if W remains at rest, V may be removed; when X has been brought into contact with W, W may be removed; and so on, until contact with B is secured. However, when it comes to proving that a connection between A and B can *not* be effected by a given combination of solids, this method is cumbersome; for the removed solids must be again and again replaced, in order to test whether some new mode of contact may not make it possible for the connection to be secured.

In the fourth place, a solid that has been removed according to the method above described may be used again, either at once or later, so that one or more solids may be used any number of times. If, in connecting A and B, V has been used m times, W n times, X p times, *etc.*, then, no matter how the order is changed, it is always possible to connect A and B by using the intervening solids the same numbers of times. And if C and D can not be connected in this way, then there is no proportion in which any collection of solids can be used to connect C and D, in which they can not also be made to connect A and B.

In particular, any two solids may be used alternately, as often as desired, in effecting a connection.

We may repeat here, with reference to the formation of chains of solids, and their manipulation, a remark that was made above with reference to the manipulation of solids as such. The number of "links" which we can manage varies greatly both with the links that are employed and the conditions under which we employ them. The inductions of geometry are based upon observations made under conditions where we are able to use any and all links that we wish to use, as often as we please; that is to say, where, for the purposes of the observation, no limitations are felt. Accordingly, we assume that any solids whatsoever may be combined in a chain, each of them being reinserted as often as we please. Here again geometry does not differ from other empirical sciences.

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[4] This brief and convenient expression will be employed frequently.

We are now prepared for the following definitions:

I^a If A, B, C, and D are such that a solid (or chain of solids) X can be found, which can connect A and B but not C and D; then C and D are said to be *farther apart than* A and B.	I^b If the solid (or chain of solids) M and the solid (or chain of solids) N are such that two solids, X and Y, can be found, which M can connect, while N can not; then M is said to be *longer than* N.

Thus we have brought before us the "indefinable" relation assumed by metrical geometry in its deductive form—not, however, as subsisting between pairs of points (of which, as yet, we know nothing), but as subsisting between pairs of solids. At the same time, there is introduced the relation "to be longer than," which we had no occasion to note in the earlier discussion, but which stands in a curious correspondence with the relation "to be farther apart than." This correspondence we shall continue to indicate by means of parallel columns.

From the fundamental induction we may at once conclude:

If A and B are farther apart than C and D, C and D are not farther apart than A and B.	If M is longer than N, N is not longer than M.
If A and B are farther apart than C and D, and C and D are farther apart than E and F; then A and B are farther apart than E and F.	If M is longer than N, and N is longer than P; then M is longer than P.
II^a If A and B are not farther apart than C and D, and C and D are not farther apart than A and B; then A and B are said to be *just as far apart as* C and D.	II^b If M is not longer than N, and N is not longer than M; then M is said to be *just as long as* N.
III^a The *distance between* A and B is the class of pairs of solids that are just as far apart as A and B (or the property of being just as far apart as A and B).	III^b The *length of* M is the class of solids (or chains of solids) that are just as long as M (or the property of being just as long as M).

The double form of these last two definitions has been sufficiently explained in another connection.[5]

IV^a If *A* and *B* are farther apart than *C* and *D*, the distance between *A* and *B* is said to be *greater than* the distance between *C* and *D*.

IV^b If *M* is longer than *N*, the length of *M* is said to be *greater than* the length of *N*.

It is easily proved that the relation "greater-than," as thus subsisting between distances on the one hand, and between lengths on the other, is asymmetrical and transitive.

We must now pass on to consider the relations that may subsist between a distance and a length:

V^a If *M* can connect *A* and *B*, but can not connect any pair of solids that are farther apart than *A* and *B;* then the distance between *A* and *B* is said to be *equal to* the length of *M*.

V^b If *M* can connect *A* and *B*, but no solid (or chain of solids) *N*, such that *M* is longer than *N*, can connect *A* and *B;* then the length of *M* is said to be *equal to* the distance between *A* and *B*.

It will be recalled, that, treating distances and lengths as classes, we do not speak of one distance's being equal to another distance, nor of one length's being equal to another length, but of identical lengths and identical distances. But a distance and a length can not be identical; hence the necessity of the above definitions.

The two following propositions are easily established:

If the distance between *A* and *B* is equal to the length of *M*, the length of *M* is equal to the distance between *A* and *B*.

If the length of *M* is equal to the distance between *A* and *B*, the distance between *A* and *B* is equal to the length of *M*.

VI^a If the distance between *A* and *B* is greater than a distance that is equal to the length of *M*, it is said to be *greater than* the length of *M*.

VI^b If the length of *M* is greater than a length that is equal to the distance between *A* and *B*, it is said to be *greater than* the distance between *A* and *B*.

It will be observed that up to this point our account of distances and of lengths has been perfectly symmetrical. At this point the symmetry breaks down.

VII^b The *sum of* the lengths of *M* and *N* is the length of the chain consisting of *M* and *N*.

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[5] See the remarks on Definition II in the preceding article.

This definition can not be duplicated for distances, for the simple reason that while there are chains of solids, there are no analogous chains of pairs of solids; or, as we may express the matter, while there are chains of lengths, we are acquainted with no chains of distances. Hence the sum of two distances must be defined indirectly, in terms of the sum of the corresponding lengths:

VII^a The *sum of* the distances between *A* and *B* and between *C* and *D* is the distance that is equal to the sum of the lengths that are equal respectively to the distance between *A* and *B* and the distance between *C* and *D*.

The importance of this defect in the general symmetry will be discussed below.

The effect of Definitions V^a and V^b, and of the two propositions which follow them, is to establish a correlation between lengths and distances. The question arises whether there can be assumed to exist a distance equal to every length, and a length equal to every distance.

On account of the freedom with which many solids can be moved, it is in general possible to exhibit a pair of solids that are at a distance equal to the length of a given solid (or chain of solids). And, though this can not always be done, no reason in any instance appears to indicate that a pair of solids at the required distance from each other *may* not occur. Accordingly, we assume that there is a distance equal to every length.

The answer to the second part of the question involves the consideration of the peculiar sense in which one solid may be *part* of another solid.

It often happens that two solids can be brought into contact with each other in such a fashion that thereafter they can be manipulated, and may therefore be regarded, as a single solid. In such a case, the collection of two solids is no mere logical collection, or class. It is a collection, between the members of which a relation subsists which makes it a "unity," or "complex"; and this unity, or complex, is a solid. The two members of the collection are then called *parts* of the collection considered as a solid; and the latter is called the *whole.*

The like may happen with any number of solids.

It also happens that a solid may be, as we say, *broken,* that is, undergo the reverse process: being changed into a collection of solids that is not itself a single solid. When this appears possible, we may, independently of any such actual process, regard the solid as a complex of those solids into which it might be broken; and these are then called its parts.

We assume that if a part of a solid is in contact with another solid, the whole is also in contact with that solid. This appears to be implied in the notion of contact, which (it will be recalled) we have not here attempted to analyze.

Observation shows that a whole is often longer than one of its parts, though the part may be just as long as the whole. But a part can not be longer than the whole. It is generally possible to break up a solid into parts (and it is thus legitimate to consider it as already made up of parts) such that the whole is longer than any one of them; and this process may, in general, be continued until the parts are such that an assigned solid is longer than any one of them. Similarly, if a pair of solids are not in contact with each other, it is generally possible to break a third solid into parts, no one of which can connect the pair. Furthermore, though it often happens that, with the available means of manipulation, a solid is unbreakable, it also often becomes possible to break such a solid; and no absolute limit to the process of continuous breaking is known. Accordingly, we assume: (1) that if M and N be each any solid, M may be considered as made up of parts such that N is longer than any one of them; and, similarly, (2) that if M be any solid, and if A and B be any pair of solids that are not in contact with each other, M may be considered as made up of parts no one of which can connect A and B.

Observation further shows that if a solid M is longer than a solid N, it is in general possible to divide M into two parts, one of which is just as long as N; and that if the length of M is greater than the distance between two solids, A and B, which are not in contact with each other, it is in general possible to divide M into two parts, the length of one of which is equal to the distance between A and B. And, again, though it often happens that with the available means of manipulation such a division of M can not be effected, some addition to these means often makes the operation possible; and we see no final limit to such possibilities. We therefore assume that if M be longer than N, it contains a part that is just as long as N; and that if its length is greater than the distance between A and B (which are not in contact with each other), it contains a part which is equal to that distance.

A similar observation applies to combinations of solids. If the length of a given chain is greater than a given distance, it is, in general, possible, by omitting one or more members of the chain, or by including only a part of one member, or both, to find a length equal to the given distance; and we accordingly assume that this is always possible.

Finally, if *A* and *B* be any pair of solids, and if *V, W, X, etc.,* be any collection of two or more solids, it is, in general, possible to form a chain of these latter solids (repeating them as often as may be desired) that can connect *A* and *B*. And though we are not always able actually to effect the connection—because of the inaccessibility of one or both of the pair of solids, or because of the difficulty of manipulating any of the members of the collection—we assume that there is no essential impossibility in the matter, that is to say, none that is not relative to our limited powers; and, accordingly, we hold that from any collection of two or more solids a chain can be formed that will connect any given pair of solids.[6]

It appears from the foregoing, that for every length there is an equal distance, and—if we leave out of consideration pairs of solids that are in contact with each other—for every distance there is an equal length. It remains to consider the one case of which exception has been made. Is there a length corresponding to the distance between two solids that are in contact with each other?

Our experience is (1) that any solid will connect a pair of solids that are in contact, and further (2) that there is no solid that will not suffice to connect some pairs of solids that are not in contact. But this is as much as to say that, in our experience, there is no solid whose length is equal to the distance between two solids that are in contact.

Nothing, however, prevents us from assuming that, *beyond the limits of our experience,* such solids exist. It might be supposed that an assumption of this character would be perfectly idle and frivolous; but that is not necessarily the case. There are two common classes of motives that lead to such assumptions. What can not possibly be perceived may be assumed to exist, first, in order to account by its behavior for the behavior of various things which we can perceive; or, secondly, in order to simplify our mode of statement of many generalizations regarding things which we can perceive. It is, indeed, not always clear which of these two classes a given assumption illustrates. The chemical atom has sometimes been regarded as a causal agent, sometimes as a fiction of merely notational significance.

[6] If the collection consists of two solids, and if these are of the same length, this principle reduces to the so-called "principle of Archimedes," that the ratio between any two distances is finite.

Let us postpone the question of motive—except as the attractiveness of an obvious symmetry may suggest itself—and arbitrarily assume that solids whose length is equal to the distance between solids that are in contact with each other exist; and, in particular, that whenever two other solids are in contact with each other, at least one of these supposititious solids is in contact with both. We have then arrived at the conception of the *point,* as it enters into metrical geometry.

As so conceived, the point has several noteworthy properties.

1. Since it can connect no solids that are not already in contact with each other, the point does not increase the length of a chain to which it is added. In other words the length of the point is *zero.*

2. The point can contain no parts which are not themselves points in contact with each other. For no part can be longer than the whole. Furthermore, every part must be in contact with every solid with which any part is in contact. Thus no part of a point can be distinguished in any way from any other part or from the whole. We therefore assume that the point has no parts.

3. Similarly, two points that are in contact with each other—*coincident* points—can have no different properties, unless because of some difference in movement. If we assume that points, like other solids, can move, we need the conception of coincident points; otherwise not.

4. Since points are not given in experience, we can not distinguish between an actual and a possible point. If we assume that points exist, every possible point must be regarded as actual.

5. Accordingly, if we assume that points exist, we must assume at least one for every possible contact between solids. In particular, every two adjacent parts of any solid call for at least one point; and, as the solid is divisible into parts *ad infinitum,* every solid must, in this sense, "contain" an infinite number of points.

The points that are thus requisite are indicated by the existence-axioms of geometry.

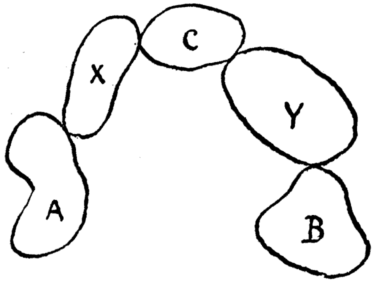
6. Points group themselves in what are called "surfaces" and "lines" (or, as the mathematicians call them, "curves"). A collection of points at which two solids may be in contact with each other is called a "surface." A surface is thus, as we say, the "outside" (or a part of the outside) of a possible solid. A collection of points at which three solids may be in contact is called a "line," or "curve." A surface, in this sense, *may* consist of a single line, or even of a single point; and a line may consist of a single point.

But why should we assume that points exist? What reason have we for entertaining a supposition, outrunning, as it does, the limits of our experience?

In a great many of the measurements in which we are interested, the distances directly involved are much greater than the lengths. This is necessarily the case, for the reason that so many important solids are too big for us to move them at all, or, at any rate, to move them freely. In such a case we measure the distance between two parts of the solid, or the distance between two other solids which the solid in question touches. For we can measure great distances with comparative ease by means of chains of solids, the separate lengths of which are of convenient magnitude.

The distances with which we have to deal are often *so much greater* than the lengths of the objects between which they are measured, that the lengths are altogether negligible in comparison. Thus we may measure the distance between two little stakes at opposite sides of a valley.

Where the lengths of the solids are altogether negligible, a considerable simplification of some of the relations involved may occur. The following case is typical:



When a chain of three solids, *X*, *C*, and *Y*, connects a pair of solids, *A* and *B*, we know that the distance between these latter solids is not greater than the length of the chain. Moreover, the distance between *A* and *C* is not greater than the length of *X*; and the distance between *C* and *B* is not greater than the length of *Y*. It follows that the distance between *A* and *B* is not greater than the sum of (1) the distance between *A* and *C*, (2) the length of *C*, and (3) the distance between *C* and *B*. Now, if the length of *C* is negligible, we may simplify this statement, saying that the distance between *A* and *B* is not greater than the sum of the distance between *A* and *C* and the distance between *C* and *B*. This need not be true, but it will not, at any rate, be seriously false.

If, now, we suppose *C* to be a point, the simplified statement becomes universally and absolutely true. And if *A* and *B* are likewise points, we may say that the distance between any two of the three points is not greater than the sum of their distances from the third point.

It may be recalled that in a previous article we used this relation as the basis of a definition of the sum of two distances. But without points the direct addition of distances is impossible: the sum of two distances is merely the distance equal to the sum of the two lengths that are severally equal to the given distances. Without points we can not have a *chain of distances,* as we have chains of lengths.[7]

With points we can have precisely that: the distances between successive members of a series of points.

But what is there of real value in this simplification? Is it not over-simplification? Since, so far as we know, there are no points, what sense is there in laying down propositions that are true only of points?

In answer we may refer to the fact already mentioned, that, though there are no points in our experience, it frequently happens that the solids with which we have to deal are relatively so small that their length does not matter to us. The condition, "Let A, B, and C be three points," is, to be sure, a condition which we have never found realized. But we find what, to us, are very close approaches to it; and, *the nearer the approach, the smaller the margin of possible error* in the conclusion, "AB is not greater than the sum of AC and CB." The geometrical formula is descriptive, not of any object of our actual experience, but of *an ideally simplified object, which the real objects may approach indefinitely.*

It is interesting to recall that Galileo has made a similar observation with regard to the principles of mechanics. Consider, for example, the "law of falling bodies": that they fall with a velocity proportional to the time that they have been falling. Is this law obeyed by the bodies that we perceive? Evidently not by the falling leaf or feather. But many other bodies obey it very closely—some so closely that it is difficult or impossible to discover any departure from exactness. But that does not in itself excuse the obvious exceptions. The excuse for the exceptions is that they do not conform to the essential conditions of the law. Their fall is interfered with by the resistance of the air; and the law requires that it shall take place in a vacuum, where no such interference is possible. But in the more favorable instances does the object fall in a vacuum? No. We have no means of obtaining a perfect vacuum. Of what value, then, is the law, since it calls for a condition that is never in our experience verified?

The answer is that, although we can not wholly eliminate the resistance of the medium through which the body falls, we can reduce this resistance in various ways. Some bodies are of such density and of such a shape, that they encounter a very slight resistance; and approximate vacuums can be formed in which even a feather falls like a shot. And the more nearly the required condition is satisfied, the more nearly the law is found to be obeyed—the smaller is the margin of error within which a prediction according to the law can be safely made. While, therefore, the law is not exactly descriptive of any observable event, it does describe *an ideally simple event which the observed events approach as a limit.* As Galileo pointed out, this is the general character of mechanical laws.

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[7] *Cf.* p. 428.

The closely analogous case of the law of the pendulum—that the period of oscillation is independent of the amplitude, and varies as the square of the length of the pendulum—is worth noticing. Apply this law to the swinging chandeliers, and it is verified, but only approximately. As before, we have to observe that the conditions laid down by the law are not adequately met. The chandelier is not a pendulum. The pendulum, for instance, swings from a point, or from a straight line perpendicular to the plane of oscillation; the chandelier does not hang from a point or even from a straight line. But where is such a pendulum to be found? It is not to be found. The pendulum, as mechanics defines it, and as the law of the pendulum describes it, is a thing lying altogether beyond any possible experience of ours—an ideal, for which we can find approximations of varying closeness, but no exact realization.

This, as we have seen, is likewise the nature of the point, and of the system of points which we call "space." A surveyor's stake is not a point, nor is the tiny pencil-dot which he makes upon his map. But the surveyor treats them as points, and thereby commits no serious error. For the purposes of his work they are *pretty good points,* as the chandelier is a pretty good pendulum. The points in space are the perfect type—the Platonic archetype, if you choose—which the stakes and pencil-dots resemble, approximate, imitate, in whose characteristic properties they participate, and of which they are, therefore, serviceable illustrations. When, in the study of material "points," the laws of geometry are exactly verified, that means that the approximation is close enough to conceal the error. But if the verification is not exact, the laws of geometry remain unrepealed. The inexactness is attributed to the imperfection of the conditions. The geometry of stakes and pencil-dots is the geometry of points *plus* an uncertain amount of unimportant variation from the norm.

Let us now turn our attention to the principle of measurement which was discussed on an earlier page of this chapter. The constancy of the relations of possible simultaneous contact between solids is the essential precondition of all measurements of lengths and distances. And yet this constancy is, so far as we can surmise, not perfectly exemplified by any collection of solids in the universe. There are many collections that exemplify it tolerably well. The vast majority of things on the earth's surface constitutes such a collection. But, if we press our observations home, we find shifting and shrinking and expansion everywhere, and the very conception of lengths and distances seems to melt into nothingness.

What course has science followed in this connection? It has simplified its account of our experience in a manner which was strongly suggested by the great mass of our observations, and which not only explained away all past exceptions but provided in advance for all possible future exceptions. This result was accomplished by putting the principle of measurement in a form in which it clearly transcends human experience: as a proposition involving a universal existential negative and limited to a single moment of time. As we have phrased it,

"If *A, B, C, D,* and *X* be any solids, and if, at any moment, *X* can connect *A* and *B* but can not connect *C* and *D,* then, at that moment, no *Y* exists which can connect *C* and *D* but not *A* and *B.*"

The conditions laid down in this principle are never given in experience. So complex a simultaneity is unobservable. But approximations to it are found. Many things change slowly, and some are changeless so far as our available instruments and methods can attest; so that the duration and successiveness of our observations is in varying degrees unimportant. In just those degrees the principle of measurement is more closely verifiable. Once more the law of nature is found to describe an ideally simplified object: a world of hypothetical solids which are absolutely rigid and—with exceptions that we have elsewhere noted—absolutely motionless.

The assumption of the existence of perfectly rigid and motionless solids, like the assumption of the existence of points, is of a purely formal character. By no human observation can they be directly confirmed or invalidated. So far as the applications of geometry are concerned, from the simple measurement of lengths and distances onward, it matters not in the least whether solids of zero-length or solids of absolutely constant length exist at all. By means of these conceptions—these figures of speech, if you please—we are enabled to analyze and describe the relations of possible simultaneous contact between solids with the greatest economy and practical success. If these relations were in practise altogether unpredictable—if the universe were to our perception a chaos, in which everything was in irregular motion—there would be no measurement; and if the lengths which we have to deal with were seldom negligible in comparison with the distances, there would be no geometry of points. The utility of the formal assumptions depends on the approximate illustration of them in the field of actual experience.

It may be asked, how far the conclusions which we have reached agree with the well-known theory of Poincaré. The answer is that they correspond pretty closely with his conception of mechanical principles, while they disagree flatly with his conception of geometrical principles. For one of the striking features of Poincaré's theory of mathematics is that geometrical and mechanical principles stand upon an essentially different footing. The former are *conventions*, resting, not on evidence, but on their superior convenience as compared with other logically possible conventions. The latter have a *double* character; first, as empirical generalizations, based (like all such generalizations) on the approximate agreement of inexact observations; and, secondly, as mathematical equations which are exact and irrefutable, but only because, as such, they are logically insignificant identities. I have tried to show that geometrical principles too, as descriptions of an ideal limiting instance, have this double character of empirical generalizations and of identities. I have tried to show how this double character arises and wherein its utility consists. If I am right, it may be suggested[8] that Poincaré's error arose from his acceptance of the traditional view, that geometry is altogether prior to mechanics. As members of the system of mathematical sciences this is true of them. As empirical sciences of nature, it is by no means strictly true of them.

In conclusion, it may be observed—though the observation goes far beyond the proper limits of this inquiry—that a very similar account may be given of the general law of the uniformity of nature.

This law has been variously regarded as a self-evident truth, as an empirical generalization, and (since Kant) as a postulate of scientific inquiry—something that is not known to be true, and can never be proved or disproved, but must be assumed if empirical science is to proceed in the search for truth. The first hypothesis was long orthodox, but has been discredited, not only by reason of the general discredit into which axioms as such have fallen, but also by reason of the unclearness and inconsistency of its own many attempted formulations. The second hypothesis has therefore become attractive to men of an open mind; but it has been hard to see how such a principle as the uniformity of nature could be established by induction. J. S. Mill's theory, that it is based upon an unbroken enumeration of positive instances, valid because commensurate with human experience, will scarcely hold; first, because by far the greater part of our experience presents no observable uniformity, and, secondly, because a great deal of our experience presents what to *prima facie* observation are distinct breaches of uniformity. The Kantian type of theory remains; but it, in turn, labors under the difficulty of conceiving how the advantages of believing a thing are a sufficient justification for assuming it. As a matter of fact, moreover, the schools of Plato and Aristotle did not accept the principle, and yet were active in many fields of investigation. And it is not apparent why all search for uniformity would be impossible except on the supposition that no irregularity anywhere exists.

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[8] *Cf.* my review of *Science and Hypothesis*, in the *Philosophical Review*, Vol. XV, No. 6 (November, 1906), pp. 634 ff.

Is not the case similar to that of the fundamental principle of spatial measurement? As has been pointed out above, direct observation does not prove this principle. Contrary experiences abound. But these we explain away by saying that in each case some one of the solids has moved or changed in length without our observing it. The principle is thus raised above any direct refutation by experience. All possible exceptions are accounted for. But at the same time it loses its value as *information*. Little good is it, merely to know that your measurements are valid provided your measuring-stick has not swelled or shrunk and the objects measured have not changed their interrelations. What is wanted is a sufficient assurance that these things have not happened—to any serious extent.

Similarly, when irregularities in the sequence of events occur we are ready in advance with a universal method of explaining them away. Either we hold to the rule, but declare that its working was interfered with by some contrary tendency, or we admit that the rule is not absolute, but at the same time declare that an absolute rule—if we could but know it—still obtains. In this sense, the uniformity of nature by no means includes or implies the acceptance of any particular uniformity. The law of gravitation, of the indestructibility of matter, or even of the conservation of energy may be only approximations to truth; but the uniformity of nature is undisturbed. To believe in it is to be committed to nothing. It is like the churchman's belief in the verbal inspiration of the original manuscripts of the Bible.

It has been argued (*e.g.*, by Sigwart) that the principle of the uniformity of nature is an indispensable premise of every inductive argument: because we know that "the given is determined," we are able to prove that a particular mode of determination obtains. On the contrary, I can not see that the general principle strengthens in the slightest degree the argument for any particular uniformity. It might, if we were able to argue: "Some mode of determination exists; no other than this suggested mode is possible; therefore this is the one.'' But the evidence for a law is never so extensive, and it is always more positive. For this reason the further argument that is sometimes met with—that the uniformity of nature can not be inductively established, because it is an essential premise of every induction—does not appear to be sound.

Men's actual belief in the uniformity of nature shows itself in their confidence in the continued operation of the various empirically discovered laws of nature. And, historically, it is the rise of empirical science that has given the principle its secure hold upon our convictions. Its earliest recorded expression, at the dawn of science, by Heraclitus of Ephesus, was based upon the simple cosmological and physiological phenomena upon which scientific attention was then fixed—the regular cycles of the day and the year and of the generations of human life—and in particular upon that first great generalization with which science may be said to have come into being: the permanence of matter amid the multiplicity of its sensible forms. The various limitations which have been set to it by ancient and modern thinkers have, for the most part, been similarly empirical in their basis, resting now on the imperfection of material things, now on the consciousness of power, now on the unpredictability of human caprice. The ascendancy of the determinist conception is a measure of the success which the enterprise of natural science has met with; and, generally speaking, it is more or less marked, as habits of thought are more strongly marked by scientific or by religious and esthetic influences.

As I see it, the uniformity of nature is a generalization more and more strongly suggested by the extension of man's understanding of nature. It is, in effect, an analytical proposition, providing, as it does, in advance for all apparent exceptions to it. Its significance lies in the fact that it presents an ideal background upon which the world, as it is now understood by us, may be projected, so that concrete programmes may thus be formed for a better understanding. Just as we have learned to see things, not simply as extending, but as extending in space; so we have learned to see the course of events, not only as exhibiting certain uniformities, but as merged in a universal cosmos. The sequences of things, as we perceive them, are often erratic, just as the measurements taken with our common yard-sticks often fail to agree by large fractions of an inch. Even the most refined observations with the most delicate instruments do not reveal a perfect harmony, though the disagreements commonly fall within a well-established margin of error. But behind the margin of error we see Nature—and Space.

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